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DIFFERENTIATION, MIGRATION, AND MIMICRY.

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IT is difficult for the mind to grasp the advance in biological science—I use the term biology in its wide etymological, not its recently restricted sense—which has taken place since I first attended the meetings of the British Association, some forty years ago. In those days, the now familiar expressions of "natural selection," "isolation," "the struggle for existence," "the survival of the fittest," were unheard of and unknown, though many an observer was busied in culling the facts which were being poured into the lap of the philosopher who should mould the first great epoch in natural science since the days of Linnæus.

It is to the importance and value of field-observation that I would venture in the first place to direct your attention.

My predecessors in this chair have been, of recent years, distinguished men who have searched deeply into the abstrusest mysteries of physiology. Thither I do not presume to follow them. I rather come before you as a survivor of the old-world naturalist, as one whose researches have been, not in the laboratory or with the microscope, but on the wide desert, the mountain side, and the isles of the sea.

This year is the centenary of the death of Gilbert White, whom we may look upon as the father of field-naturalists. It is

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true that Sir T. Browne, Willughby, and Ray had each, in the middle of the seventeenth century, committed various observations to print; but though Willughby, at least, recognised the importance of the soft parts in affording a key to classification, as well as the osteology, as may be seen from his observation of the peculiar formation in the Divers (*Colymbidæ*) of the tibia, with its prolonged procnemial process, of which he has given a figure, or his description of the elongation of the posterior branches of the Woodpecker's tongue, as well as by his description of the intestines of all specimens which came under his notice in the flesh, none of these writers systematically noted the habits of birds, apart from an occasional mention of their nidification, and very rarely do they even describe the eggs. But White was the first observer to recognise how much may be learnt from the life habits of birds. He is generally content with recording his observations, leaving it to others to speculate. Fond of Virgilian quotations (he was a Fellow of Oriel of the last century), his quotations are often made with a view to prove the scrupulous accuracy of the Roman poet, as tested by his (White's) own observations.

In an age incredulous as to that which appears to break the uniformity of nature, but quick to recognise all the phenomena of life, a contrast arises before the mind's eye between the abiding strength of the objective method, which brings Gilbert White in touch with the great writers whose works are for all time, and the transient feebleness of the modern introspective philosophies, vexed with the problems of psychology. The modern psychologist propounds his theory of man and the universe, and we read him, and go on our way, and straightway forget. Herodotus and Thucydides tell a plain tale in plain language, or the Curate of Selborne shows us the hawk on the wing, or the snake in the grass, as he saw them day by day, and, somehow, the simple story lives, and moves him who reads it long after the subtleties of this or that philosophical theory have had their day and passed into the limbo of oblivion. But, invaluable as has been the example of Gilbert White in teaching us how to observe, his field was a very narrow one, circumscribed for the most part by the boundaries of a single parish, and on the subject of geographical distribution (as we know it now) he could contribute nothing—a subject on which even the best explorers of that day were strangely unobservant and inexact. A century and a half ago, it had not come to be recognised that distribution is, along, of course, with morphology and physiology, a most important factor in determining the facts of biology. It is difficult to estimate what might have been gained in the case of many species, now irreparably lost, had Forster and the other companions of Captain Cook—to say nothing of many previous voyagers—had the slightest conception of the importance of noting the exact locality of each specimen they collected. They seem scarcely to have recognised the specific distinctions of the characteristic genera of the Pacific Islands at all, or, if they did, to have dismissed them with the remark, "On this island was found a Flycatcher, a Pigeon, or a Parrot similar to those found in New Holland, but with white tail-feathers instead of black, an orange instead of a scarlet breast, or red shoulders instead of yellow." As we turn over the pages of Latham, or Shaw, how often do we find for locality one of the islands of the South Sea, and even, where the locality is given, subsequent research has proved it erroneous, as though the specimens had been subsequently ticketed; Le Vaillant described many of his South African birds from memory. Thus Latham, after describing very accurately *Rhipidura flabellifera*, from the south island of New Zealand, remarks, apparently on Forster's authority, that it is subject to variation; that in the island of Tanna another was met with, with a different tail, &c., and that there was another variety in the collection of Sir Joseph Banks. Endless perplexity has been caused by the *Psittacus pygmæus* of Gmelin (of which Latham's type is at Vienna) being stated in the inventory as from Botany Bay, by Latham from Otaheite, and in his book as inhabiting several of the islands of the South Seas, and now it proves to be the female *Psittacus palmarum* from the New Hebrides. These are but samples of the confusion caused by the inaccuracies of the old voyagers. Had there been in the first crew who landed on the island of Bourbon, I will not say a naturalist, but even a simple-hearted Leguat, to tell the artless tale of what he saw, or had there been among the Portuguese discoverers of Mauritius one who could note and describe the habits of its birds with the accuracy with which a Poulton could record the ways and doings of our Lepidoptera, how vastly would our knowledge of a perished fauna have been enriched! It is only since we learned from Darwin and Wallace the power of isolation in the differentiation of species, that special attention has been paid to the peculiarities of insular forms. Here the field-naturalist comes in as the helpful servant of the philosopher and the systematist, by illustrating *the operation of isolation in the differentiation of species.* I may take the typical examples of two groups of oceanic islands, differing as widely as possible in their position on the globe, the Sandwich Islands in the centre of the Pacific, thousands of miles from the nearest Continent, and the Canaries, within sight of the African coast; but agreeing in this, that both are truly oceanic groups, of purely volcanic origin, the ocean depths close to the Canaries, and between the different islands, varying from 1500 to 2000 fathoms. In the one we may study the expiring relics of an avifauna completely differentiated by isolation; in the other we have the opportunity of tracing the incipient stages of the same process.

The Sandwich Islands have long been known as possessing an avifauna not surpassed in interesting peculiarity by that of New Zealand or Madagascar; in fact, it seems as though their vast distance from the continent had intensified the influences of isolation. There is scarcely a passerine bird in its indigenous fauna which can be referred to any genus known elsewhere. But, until the very recent researches of Mr. Scott Wilson, and the explorations of the Hon. W. Rothschild's collectors, it was not known that almost every island of the group possessed one or more representatives of each of these peculiar genera. Thus, every island which has been thoroughly explored, and in which any extent of the primeval forest remains, possesses, or has possessed, its own peculiar species of *Hemignathus*, *Himatione*, *Phæornis*, *Acrulocercus*, *Loxops*, *Drepanis*, as well as the massive-beaked finches, which emulate the *Geospiza* of the Galapagos. Prof. Newton has shown that while the greater number of these are, probably, of American origin, yet the South Pacific has contributed its quota to this museum of ornithological rarities, which Mr. C. B. Clarke (Phil. Trans. 1892, pp. 371—387) very justly proposes to make a distinct biological sub-region.

That each of the islands of this group, however small, should possess a flora specifically distinct, suggests thoughts of the vast periods occupied in their differentiation.

In the Canary Islands, either because they are geologically more recent, or because of their proximity to the African coast, which has facilitated frequent immigrations from the continent, the process of Differentiation is only partially accomplished. Yet there is scarcely a resident species which is not more or less modified, and this modification is yet further advanced in the westernmost islands than in those nearest to Africa. In Fuertaventura and Lanzarote, waterless and treeless, there is little change, and the fauna is almost identical with that of the neighbouring Sahara. There is a Whinchat, *Pratincola dacotiæ*, discovered by my companion, Mr. Meade-Waldo, peculiar to Fuertaventura, which may possibly be found on the opposite coast, though it has not yet been met with by any collectors there. Now, our Whinchat is a common winter visitant all down the West African coast, and it seems probable that isolation has produced the very marked characters of the Canarian form, while the continental individuals have been restrained from variation by their frequent association with their migratory relations. A similar cause may explain why the Blackbird, an extremely common resident in all the Canary Islands, has not been modified in the least, since many migratory individuals of the same species sojourn every winter in these islands. Or take the Blue Titmouse, Our familiar resident is replaced along the coast of North Africa by a representative species, *Parus ultramarinus*, differentiated chiefly by a black instead of a blue cap, and a slate-coloured instead of a green back. The Titmouse of Lanzarote and Fuertaventura is barely separable from that of Algeria, but is much smaller and paler, probably owing to scarcity of food and a dry desert climate. Passing, 100 miles further to sea, to Grand Canary, we find in the woods and forests a bird in all respects similar to the Algerian in colour and dimensions, with one exception—the greater wing-coverts of the Algerian are tipped with white, forming a broad bar when the wing is closed. This, present in the Fuertaventura form, is represented in the Canarian by the faintest white tips, and in the birds from the next islands. Tenerife and Gomera, this is altogether absent. This form has been recognised as *P. tenerifæ*. Proceeding to the north-west outermost island, Palma, we find a very distinct species, with different proportions, a longer tail, and white abdomen instead of yellow. In the Ultima Thule, Hierro, we find a second very distinct species, resembling that of Tenerife in the absence of the wing bar and in all other respects, except that the back is green like the European, instead of slate as in all the other species. Thus we find in this group a uniform graduation of variation as we proceed further from the cradle of the race.

A similar series of modifications may be traced in the Chaffinch (*Fringilla*), which has been in like manner derived from the North African *F. spodiogena*, and in which the extreme variation is to be found in the westernmost islands of Palma and Hierro. The Willow Wren, *Phylloscopus trochilus*, extremely numerous and *resident*, has entirely changed its habits, though not its plumage, and I have felt justified in distinguishing it as *P. fortunatus*. In note and habits it is entirely different from our bird, and though it builds a domed nest it is always near the top of lofty trees, most frequently in palm-trees. The only external difference from our bird consists in its paler tarsi and more rounded wing, so that its power of flight is weaker, but, were it not for the marked difference in its habits and voice, I should have hesitated to differentiate it. In the Kestrel and Great Spotted Woodpecker there are differences which suggest incipient species, while the forests of the wooded western islands yield two very peculiar Pigeons, differing entirely from each other in their habits, both probably derived from our Wood Pigeon, but even further removed from it than the *Columba trocaz* of Madeira, and by their dark chestnut coloration, suggesting that peculiar food, in this case the berries of the tree laurel, has its full share in the differentiation of isolated forms. If we remember the variability of the pigments in the food of birds, and the amount absorbed and transferred to the skin and plumage, the variability in the tints and patterns of many animals can be more readily understood.

One other bird deserves notice, the *Caccabis*, or Red-legged Partridge, for here, and here alone, we have chronological data. The Spaniards introduced *Caccabis rufa* into Canary, and *C. petrosa* into Tenerife and Gomera, and they have never spread from their respective localities. Now, both species, after a residence of only 400 years, have become distinctly modified. *C. rufa* was introduced into the Azores also, and changed exactly in the same manner, so much so that Mr. Godman, some years ago, in his 'Natural History of the Azores,' would have described it as distinct, but that the only specimen he procured was in moult and mutilated, and it now proves identical with the

Canarian bird. Besides minor differences, the beak is one-fourth stouter and longer than in the European bird, the tarsus very much stouter and longer, and the back is grey rather than russet. The grey back harmonises with the volcanic dark soil of the rocks of the Canaries, as the russet does with the clay of the plains of England and France. In the Canaries the bird lives under different conditions from those of Europe. It is on the mountain sides and among rocks that the stouter beak and stronger legs are indispensable to its vigorous existence. It is needless to go into the details of many other species. We have here the effect of changed conditions of life in 400 years. What may they not have been in 400 centuries? We have the result of peculiar food in the Pigeons, and of isolation in all the cases I have mentioned. Such facts can only be supplied to the generaliser and the systematist through the accurate and minute observations of the field-naturalist.

The character of the avifauna of the Comoro Islands, to take another insular group, seems to stand midway in the differentiating process between the Canaries and the Sandwich Islands. From the researches of M. Humblot, worked out by MM. Milne-Edwards and Oustalet, we find that there are twenty-nine species acknowledged as peculiar; two species from South Africa and twenty-two from Madagascar in process of specification, called by M. Milne-Edwards secondary or derived species.

The little Christmas Island, an isolated rock 200 miles south of Java, only twelve miles in length, has been shown by Mr. Lister to produce distinct and peculiar forms of every class of life, vegetable and animal. Though the species are few in number, yet every mammal and land bird is endemic; but, as Darwin remarks, to ascertain whether a small isolated area, or a large open area like a continent, has been more favourable for the production of new organic forms, we ought to make the comparison between equal times, and this we are incapable of doing. My own attention was first directed to this subject when, in the year 1857–58, I spent many months in the Algerian Sahara, and noticed the remarkable variations in different groups, according to elevation from the sea, and the difference of soil and vegetation. The 'Origin of Species' had not then appeared; but on my return my attention was called to the communication of Darwin and Wallace to the Linnean Society* on the tendencies of species to form varieties, and on the perpetuation of varieties and species by means of natural selection. I then wrote ('Ibis,' 1859, pp. 429—433):—"It is hardly possible, I should think, to illustrate this theory better than by the Larks and Chats of North Africa. In all these, in the congeners of the Wheatear, of the Rock Chat, of the Crested Lark, we trace gradual modifications of coloration and of anatomical structure, deflecting by very gentle gradations from the ordinary type, but, when we take the extremes, presenting the most marked differences. . . . . In the desert, where neither trees, brushwood, nor even undulations of surface, afford the slightest protection to an animal from its foes, a modification of colour, which shall be assimilated to that of the surrounding country, is absolutely necessary. Hence, without exception, the upper plumage of every bird—whether Lark, Chat, Sylvia, or Sand Grouse—and also the fur of all the smaller mammals, and the skin of all the Snakes and Lizards, is of the uniform isabelline or sand-colour. It is very possible that some further purpose may be served by the prevailing colours, but this appears of itself a sufficient explanation. There are individual varieties of depth of hue among all creatures. In the struggle for life which we know to be going on among all species, a very slight change for the better, such as improved means of escape from its natural enemies (which would be the effect of an alteration from a conspicuous colour to one resembling the hue of the surrounding objects), would give the variety that possessed it a decided advantage over the typical or other forms of the species. . . . . To apply the theory to the case of the Sahara. If the Algerian Desert were colonised by a few pairs of Crested Larks—putting aside the ascertained fact of the tendency of an arid, hot climate to bleach all dark colours—we know that the probability is that one or two pairs would be likely to be of a darker complexion than the others. These, and such of their offspring as most resembled them, would become more liable to capture by their natural enemies, hawks and carnivorous beasts. The lighter-coloured ones would enjoy more or less immunity from such attacks. Let this state of things continue for a few hundred years and the dark-coloured individuals would be exterminated, the light-coloured remain and inherit the land. This process, aided by the above-mentioned tendency of the climate to bleach the coloration still more, would in a few centuries produce the *Galerita abyssinica* as the typical form;

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* Journ. Linn. Soc. vol. iii. (1859), pp. 45—62.

and it must be noted that between it and the European *G. cristata* there is no distinction but that of colour.

"But when we turn to *Galerita isabellina*, *G. arenicola*, and *G. macrorhyncha*, we have differences not only of colour, but of structure. These differences are most marked in the form of the bill. Now, to take the two former first, *G. arenicola* has a very long bill, *G. isabellina* a very short one; the former resorts exclusively to the deep, loose, sandy tracts, the latter haunts the hard and rocky districts. It is manifest that a bird whose food has to be sought for in deep sand derives a great advantage from any elongation, however slight, of its bill. The other, which feeds among stones and rocks, requires strength rather than length. We know that even in the type species the size of the bill varies in individuals—in the Lark as well as in the Snipe. Now, in the desert, the shorter-billed varieties would undergo comparative difficulty in finding food where it was not abundant, and consequently would not be in such vigorous condition as their longer-billed relations. In the breeding season, therefore, they would have fewer eggs and a weaker progeny. Often, as we know, a weakly bird will abstain from matrimony altogether. The natural result of these causes would be that in course of time the longest-billed variety would steadily predominate over the shorter, and, in a few centuries, they would be the sole existing race; their shorter-billed fellows dying out until that race was extinct. The converse will still hold good of the stout-billed and weaker-billed varieties in a rocky district.

"Here are only two causes enumerated which might serve to create as it were, a new species from an old one. Yet they are perfectly natural causes, and such as I think must have occurred, and are possibly occurring still. We know so very little of the causes which, in the majority of cases, make species rare or common, that there may be hundreds of others at work, some even more powerful than these, which go to perpetuate and eliminate certain forms 'according to natural means of selection.'"

It would appear that those species in continental areas are equally liable to variation with those which are isolated in limited areas, yet that there are many counteracting influences which operate to check this tendency. It is often assumed, where we find closely allied species apparently inter-breeding at the centre of their area, that the blending of forms is caused by the two races commingling. Judging from insular experience I should be inclined to believe that the theory of inter-breeding is beginning at the wrong end, but rather that while the generalised forms remain in the centre of distribution, we find the more decidedly distinct species at the extremes of the range, caused not by inter-breeding, but by differentiation. To illustrate this by the group of the Blue Titmouse. We find in Central Russia, in the centre of distribution of the family, the most generalised form, *Parus pleskii,* partaking of the characters of the various species east, west, and south. In the north-east and north it becomes differentiated as *P. cyaneus;* to the south-west and west into *P. cæruleus* and its various sub-species, while a branch extending due east has assumed the form of *P. flavipectus,* bearing traces of affinity to its neighbour *P. cyaneus* in the north, which seems evidently to have been derived from it.

But the scope of field observation does not cease with geographical distribution and modification of form. The closet systematist is very apt to overlook or to take no count of habits, voice, modification, and other features of life which have an important bearing on the modification of species. To take one instance, the Short-toed Lark (*Calandrella brachydactyla*) is spread over the countries bordering on the Mediterranean; but, along with it, in Andalusia alone is found another species, *C. bætica,* of a rather darker colour, and with the secondaries generally somewhat shorter. Without further knowledge than that obtained from a comparison of skins, it might be put down as an accidental variety. But the field naturalist soon recognises it as a most distinct species. It has a different voice, a differently shaped nest; and, while the common species breeds in the plains, this one always resorts to the hills. The Spanish shepherds on the spot recognise their distinctness, and have a name for each species. Take, again, the eastern form of the common Song Thrush. The bird of North China, *Turdus auritus,* closely resembles our familiar species, but is slightly larger, and there is a minute difference in the wing formula. But the field naturalist has ascertained that it lays eggs like those of the Missel Thrush, and it is the only species closely allied to our bird which does not lay eggs of a blue ground colour. The Hedge Accentor of Japan (*Accentor rubidus*) is distinguished from our most familiar friend, *A. modularis*, by delicate difference of hue. But, though in gait and manner it closely resembles it, I was surprised to find the Japanese bird strikingly distinct in habits and life, being found only in forest and brushwood several thousand feet above the sea. I met with it first at Chiusenze—6000 feet—before the snow had left the ground, and in summer it goes higher still, but never descends to the cultivated land. If both species are derived, as seems probable, from *A. immaculatus* of the Himalayas, then the contrast in habits is easily explained. The lofty mountain ranges of Japan have enabled the settlers there to retain their original habits, for which our humbler elevations have afforded no scope.

On the solution of the problem of *the Migration of birds*, the most remarkable of all the phenomena of animal life, much less aid has been contributed by the observations of field naturalists than might reasonably have been expected. The facts of migration have, of course, been recognised from the earliest times, and have afforded a theme for Hebrew and Greek poets 3000 years ago. Theories which would explain it are rife enough, but it is only of late years that any systematic effort has been made to classify and summarise the thousands of data and notes which are needed in order to draw any satisfactory conclusion. The observable facts may be classified as to their bearing on the whither, when, and how of migration, and after this we may possibly arrive at a true answer to the Why? Observation has sufficiently answered the first question, Whither?

There are scarcely any feathered denizens of earth or sea to the summer and winter ranges of which we cannot now point. Of almost all the birds of the holoarctic fauna, we have ascertained the breeding-places and the winter resorts. Now that the Knot and the Sanderling have been successfully pursued even to Grinnell Land, there remains but the Curlew Sandpiper (*Tringa subarquata*), of all the known European birds, whose breeding-ground is a virgin soil, to be trodden, let us hope, in a successful exploration by Nansen, on one side or other of the North Pole. Equally clearly ascertained are the winter quarters of all the migrants. The most casual observer cannot fail to notice in any part of Africa, north or south, west coast or interior, the myriads of familiar species which winter there. As to the time of migration, the earliest notes of field-naturalists have been the records of the dates of arrival of the feathered visitors. We possess them for some localities, as for Norfolk by the Marsham family, so far back as 1736. In recent years these observations have been carried out on a larger and more systematic scale, by Middendorff, who, forty years ago, devoted himself to the study of the lines of migration in the Russian Empire, tracing what he called the *isopipteses*, the lines of simultaneous arrival of particular species, and by Professor Palmén, of Finland, who, twenty years later, pursued a similar course of investigation; and by Professor Baird on the migration of North American birds; and subsequently by Severtzoff as regards Central Asia, and Menzbier as regards Eastern Europe. As regards our own coasts, a vast mass of statistics has been collected by the labours of the Migration Committee appointed by the British Association in 1880, for which our thanks are chiefly due to the indefatigable zeal of Mr. J. Cordeaux, and his colleague Mr. J. A. Harvie Brown, the originators of the scheme by which the lighthouses were for nine years used as posts of observation on migration. The reports of that Committee are familiar to us, but the inferences are not yet worked out. I cannot but regret that the Committee has been allowed to retire. Professor W. W. Cooke has been carrying on similar observations in the Mississippi Valley, and others, too numerous to mention, have done the same elsewhere. But, as Professor Newton has truly said, all these efforts may be said to pale before the stupendous amount of information amassed during more than fifty years by the venerable Herr Gätke of Heligoland, whose work we earnestly desire may soon appear in an English version.

We have, through the labours of the writers I have named, and many others, arrived at a fair knowledge of the When? of migration. Of the How? we have ascertained a little, but very little. The lines of migration vary widely in different species, and in different longitudes. The theory of migration being directed towards the magnetic pole, first started by Middendorff, seems to be refuted by Baird, who has shown that in North America the theory will not hold. Yet, in some instances, there is evidently a converging tendency in northward migrations. The line, according to Middendorff, in Middle Siberia is due north, in Eastern Siberia S.E. to N.W., and in Western Siberia from S.W. to N.E. In European Russia Menzbier traces four northward routes: (1) A coast line coming up from Norway round the North

Cape to Nova Zembla. (2) The Baltic line with bifurcation, one proceeding by the Gulf of Bothnia, and the other by the Gulf of Finland, which is afterwards again subdivided. (3) A Black Sea line, reaching nearly as far north as the valley of the Petchora. (4) The Caspian line, passing up the Volga, and reaching as far east as the valley of the Obi by other anastomosing streams.

Palmén has endeavoured to trace the lines of migration on the return autumnal journey in the eastern hemisphere, and has arranged them in nine routes: (1) From Nova Zembla, round the West of Norway, to the British Isles. (2) From Spitzbergen, by Norway, to Britain, France, Portugal, and West Africa. (3) From North Russia, by the Gulf of Finland, Holstein, and Holland, and then bifurcating to the west coast of France on the one side, and on the other up the Rhine to Italy and North Africa. (4 *a*) Down the Volga by the Sea of Azof, Asia Minor, and Egypt, while the other portion (4 *b*), trending east, passes by the Caspian and Tigris to the Persian Gulf. (5) By the Yenesei to Lake Baikal and Mongolia. (6) By the Lena on to the Amoor and Japan. (7) From East Siberia to the Corea and Japan. 8) Kamschatka to Japan and the Chinese coast. (9) From Greenland, Iceland, and the Faroes, to Britain, where it joins line 2.

All courses of rivers of importance form minor routes, and consideration of these lines of migration might serve to explain the fact of North American stragglers, the waifs and strays which have fallen in with great flights of the regular migrants, having been more frequently shot on the east coast of England than on the west coast, or in Ireland. They have not crossed the Atlantic, but have come from the far north, where a very slight deflection east or west might alter their whole course, and in that case they would naturally strike either Iceland or the west coast of Norway, and in either case would reach the east coast of Britain. But, if by storms, and the prevailing winds of the North Atlantic coming from the west, they had been driven out of their usual course, they would strike the coast of Norway, and so find their way hither in the company of their congeners.

As to the elevation at which migratory flights are carried on, Herr Gätke, as well as many American observers, holds that it is generally far above our ken, at least in normal conditions of the atmosphere, and that the opportunities of observation, apart from seasons and unusual atmospheric disturbance, are confined chiefly to unsuccessful and abortive attempts. It is maintained that the height of flight is some 1500 to 15,000 feet; and if this be so, as there seems every reason to admit, the aid of land bridges and river valleys becomes of very slight importance. A trivial instance will illustrate this. There are two species of Blue-throat, *Cyanecula suecica* and *C. leucocyana:* the former with its red breast patch is abundant in Sweden in summer, but is never found in Germany, except most accidentally, as the other is the common form of Central Europe. Yet both are abundant in Egypt and Syria, where they winter, and I have, on several occasions, obtained both species out of the same flock. Hence we infer that the Swedish bird makes its journey from its winter quarters with scarely a halt, while the other proceeds leisurely to its nearer summer quarters. On the other hand, I have more than once seen myriads of Swallows, Martins, Sand Martins, and, later in the season, Swifts, passing up the Jordan Valley and along the Bukaa of Central Syria, at so slight an elevation that I was able to distinguish at once whether the flight consisted of Swallows or House Martins. This was in perfectly calm clear weather. One stream of Swallows, certainly not less than a quarter of a mile wide, occupied more than half an hour in passing over one spot; and flights of House Martins, and then of Sand Martins, the next day, were scarcely less numerous. These flights must have been straight up from the Red Sea, and may have been the general assembly of all those which had wintered in East Africa. I cannot think that these flights were more than 1000 feet high. On the other hand, when standing on the highest peak in the Island of Palma, 6500 feet, with a dense mass of clouds beneath us, leaving nothing of land or sea visible, save the distant peak of Tenerife, 13,000 feet, I have watched a flock of Cornish Choughs soaring above us, till at length they were absolutely undistinguishable by us except with field-glasses.

As to the speed with which the migration flights are accomplished, they require much further observation. Herr Gätke maintains that Godwits and Plovers can fly at the rate of 240 miles an hour (!), and the late Dr. Jerdon stated that the Spine-tailed Swift (*Acanthyllis caudacutus*), roosting in Ceylon, would reach the Himalayas (1200 miles) before sunset. Certainly in their ordinary flight the Swift is the only bird I have ever noticed to outstrip an express train on the Great Northern Railway.

Observation has shown us that, while there is a regular and uniform migration in the case of some species, yet that, beyond these, there comes a partial migration of some species, immigrants and emigrants simultaneously, and this, besides the familiar vertical emigration from higher to lower altitudes and *vice versâ*, as in the familiar instance of the Lapwing and Golden Plover. There is still much scope for the field-naturalist in observation of these partial migrations. There are also species in which some individuals migrate and some are sedentary; *e. g.* in the few primeval forests which still remain in the Canary Islands, and which are enshrouded in almost perpetual mist, the Woodcock is sedentary, and not uncommon. I have often put up the bird and seen the eggs; but in winter the number is vastly increased, and the visitors are easily to be distinguished from the residents by their lighter colour and larger size. The resident never leaves the cover of the dense forest, where the growth of ferns and shrubs is perpetual, and fosters a moist, rich, semi-peaty soil, in which the Woodcock finds abundant food all the year, and has thus lost its migratory instincts.

But why do birds migrate? Observation has brought to light many facts which seem to increase the difficulties of a satisfactory answer to the question. The autumnal retreat from the breeding quarters might be explained by a want of sufficient sustenance as winter approaches in the higher latitudes; but this will not account for the return migration in spring, since there is no perceptible diminution of supplies in the winter quarters. A friend of mine, who was for some time stationed as missionary at Kikombo, on the high plateau south-east of Victoria Nyanza Lake, almost under the equator, where there is no variation in the seasons, wrote to me that from November to March the country swarmed with Swallows and Martins, which seemed to the casual observer to consist almost wholly of our three species, though occasionally a few birds of different type might be noticed in the larger flocks. Towards the end of March, without any observable change in climatic or atmospheric conditions, nine-tenths of the birds suddenly disappeared, and only a sprinkling remained. These, which had previously been lost amid the myriad of winter visitants, seemed to consist of four species, of which I received specimens of two, *Hirundo puella* and *H. senegalensis*. One, described as white underneath, is probably *H. æthiopica ;* and the fourth, very small and quite black, must be a *Psalidoprocne*. All these remained through spring and summer. The northward movement of all the others must be through some impulse not yet ascertained. In many other instances observation has shown that the impulse of movement is not dependent on the weather at the moment. This is especially the case with sea birds. Professor Newton observes they can be trusted as the almanack itself. Foul weather or fair, heat or cold, the Puffins, *Fratercula arctica*, repair to some of their stations punctually on a given day, as if their movements were regulated by clockwork. In like manner, whether the summer be cold or hot, the Swifts leave their summer home in England about the first week in August, only occasional stragglers ever being seen after that date. So in three different years in Syria I noticed the appearance of the Common Swift (*Cypselus apus*) in myriads on one day in the first week in April. In the case of almost all the land birds, it has been ascertained by repeated observations that the male birds arrive some days before the hens. I do not think it is proved that they start earlier; but, being generally stronger than the females, it is very natural that they should outstrip their weaker mates. I think, too, that there is evidence that those species which have the most extended southerly, have also the most extended northerly range. The same may hold good of individuals of the same species, and may be accounted for by, or account for, the fact that, *e. g.*, the inividuals of the Wheatear or of the Willow Wren which penetrate furthest north have longer and stronger wings than those individuals which terminate their journey in more southern latitudes. The length of wing of two specimens of *Saxicola œnanthe* in my collection, from Greenland and Labrador, exceeds by 0·6 the length of British and Syrian specimens, and the next longest, exceeding them by 0·5 inch, is from the Gambia. So the sedentary *Phylloscopus trochilus* of the Canaries has a perceptibly shorter wing than European specimens.

To say that migration is performed by instinct is no explanation of the marvellous faculty,—it is an evasion of the difficulty. Professor Möbius holds that birds crossing the ocean may be guided by observing the rolling of the waves; but this will not hold good in the varying storms of the Atlantic, still less in the vast stretch of stormy and landless ocean crossed by the Bronze Cuckoo (*Chrysococcyx lucidus*) in its passage from New Guinea to New Zealand. Professor Palmén ascribes the due performance of the flight to experience, but this is not confirmed by field observers. He assumes that the flights are led by the oldest and strongest; but observation by Herr Gätke has shown that among migrants, as the young and old journey apart and by different routes, the former can have had no experience. All ornithologists are aware that the parent Cuckoos leave this country long before their young ones are hatched by their foster-parents. The sense of sight cannot guide birds which travel by night, or span oceans or continents in a single flight. In noticing all the phenomena of migration, there yet remains a vast untilled region for the field-naturalist.

What Professor Newton terms "the sense of direction, unconsciously exercised," is the nearest approach yet made to a solution of the problem. He remarks how vastly the sense of direction varies in human beings, contrasting its absence in the dwellers in towns compared with the power of the shepherd and the countryman, and, infinitely more, with the power of the savage or the Arab. He adduces the experience of Middendorff among the Samojeds, who know how to reach their goal by the shortest way through places wholly strange to them. He had known it among dogs and horses (as we may constantly perceive), but was surprised to find the same incomprehensible animal-faculty unweakened among uncivilised men. Nor could the Samojeds understand his enquiry, how they did it? They disarmed him by the question, How now does the Arctic Fox find its way aright on the Tundra, and never go astray? and Middendorff adds, "I was thrown back on the unconscious performance of an inherited animal-faculty"; and so are we!

There is one more kind of migration, on which we know nothing, and where the field-naturalist has still abundant scope for the exercise of observation. I mean what is called exceptional migration, not the mere wanderings of waifs and strays, nor yet the uncertain travels of some species, as the Crossbill in search of food, but the colonising parties of many gregarious species, which generally, so far as we know in our own hemisphere, travel from east to west, or from south-east to north-west. Such are the Waxwing, *Ampelis garrula*, the Pastor Starling, *Pastor roseus*, and Pallas's Sand Grouse, after an interval sometimes of many years, or sometimes for two or three years in succession. The Waxwing will overspread Western Europe in winter for a short time. It appears to be equally inconstant in its choice of summer quarters, as was shown by Mr. J. Wolley in Lapland. The Rose Pastor regularly winters in India, but never remains to breed. For this purpose the whole race seems to collect and travel north-west, but rarely, or after intervals of many years, returns to the same quarters. Verona, Broussa, Smyrna, Odessa, the Dobrudscha have all during the last half-century been visited for one summer by tens of thousands, who are attracted by the visitations of locusts, on which they feed, rear their young, and go. These irruptions, however, cannot be classed under the laws of ordinary migration. Not less inexplicable are such migrations as those of the African Darter, which, though never yet observed to the north of the African lakes, contrives to pass, every spring, unobserved to the lake of Antioch in North Syria, where I found a large colony rearing their young, and which, so soon as their progeny was able to fly, disappeared to the south-east as suddenly as they had arrived.

There is one possible explanation of "the sense of direction unconsciously exercised," which I submit as a working hypothesis. We are all aware of the instinct, strong both in mammals and birds without exception, which attracts them to the place of their nativity. When the increasing cold of the northern regions, in which they all had their origin, drove the mammals southward, they could not retrace their steps, because the increasing polar sea, as the arctic continent sank, barred the way. The birds reluctantly left their homes as winter came on, and followed the supply of food. But as the season in their new residence became hotter in summer, they instinctively returned to their birth-places, and there reared their young, retiring with them when the recurring winter impelled them to seek a warmer climate. Those species which, unfitted for a greater amount of heat by their more protracted sojourn in the northern regions, persisted in revisiting their ancestral homes, or getting as near to them as they could, retained a capacity for enjoying a temperate climate, which, very gradually, was lost by the species which settled down more permanently in their new quarters, and thus a law of migration became established on the one side, and sedentary habits on the other.

If there be one question on which the field-naturalist may contribute, as lion's provider to the philosopher, more than another, it is on the now much disputed topic of "mimicry," whether protective or aggressive. As Mr. Beddard has remarked on this subject, "The field of hypothesis has no limits, and what we need is more study"—and, may we not add, more accurate observation of facts. The theory of protective mimicry was first propounded by Mr. H. W. Bates, from his observations on the Amazon. He found that the group of butterflies, *Heliconiidæ*, conspicuously banded with yellow and black, were provided with certain glands which secrete a nauseating fluid, supposed to render them unpalatable to birds. In the same districts he found also similarly coloured butterflies, belonging to the family *Pieridæ*, which so closely resembled the others in shape and markings as to be easily mistaken for them, but which, unprovided with such secreting glands, were unprotected from the attacks of birds. This resemblance, he thought, was brought about by natural selection for the protection of the edible butterflies, through the birds mistaking them for the inedible kinds. Other cases of mimicry among a great variety of insects have since been pointed out, and the theory of protective mimicry has gained many adherents. Among birds, many instances have been adduced. Mr. Wallace has described the extraordinary similarity between birds of very different families, *Oriolus bouruensis* and *Philemon moluccensis*, both peculiar to the island of Bouru. Mr. H. O. Forbes has discovered a similar brown Oriole, *Oriolus decipiens*, as closely imitating the appearance of the *Philemon timorlauensis* of Timorlaut. A similar instance occurs in Ceram. But Mr. Wallace observes that, while usually the mimicking species is less numerous than the mimicked, the contrary appears to be the case in Bouru, and it is difficult to see what advantage has been gained by the mimicry. Now, all the species of *Philemon* are remarkably sombre-coloured birds, and the mimicry cannot be on their side. But there are other brown Orioles, very closely resembling those named, in other Moluccan islands, and yet having no resemblance to the *Philemon* of the same island, as may be seen in the case of the *Oriolus phæochromus* and *Philemon gilolensis* from Gilolo. Yet the Oriole has adopted the same livery which elsewhere is a perfect mimicry. May it not therefore be that we have, in this group of brown Orioles, the original type of the family, undifferentiated? As they spread east and south we may trace the gradation, through the brown striation of the New Guinea bird, to the brighter, green-tinged form of the West Australian and the green plumage of the Southern Australian, while westward the brilliant yellows of the numerous Indian and African species were developed, and another group, preferring higher elevations, passing through the mountain ranges of Java, Sumatra, and Borneo, intensified the aboriginal brown into black, and hence were evolved the deep reds of the various species which culminate in the crimson of Formosa, *Oriolus ardens*, and the still deeper crimsons of *O. trailli* of the Himalayas.

It is possible that there may be similarity without mimicry, and, by the five laws of mimicry as laid down by Wallace, very many suggested cases must be eliminated. We all know that it is quite possible to find between species of very different genera extraordinary similarity which is not mimetic. Take, for instance, the remarkable identity of coloration in the case of some of the African species *Macronyx* and the American *Sturnella*, or, again, of some of the African *Campephagæ* and the American *Agelæus*. The outward resemblance occurs in both cases in the red as well as in the yellow-coloured species of all four groups. But we find that the *Macronyx* of America and the *Campephagæ* of Africa, in acquiring this coloration, have departed widely from the plain colour found in their immediate relatives. If we applied Mr. Scudder's theory on insects, we must imagine that the prototype form has become extinct, while the mimicker has established its position. This is an hypothesis which it is easier to suggest than either to prove or disprove. Similar cases may frequently be found in Botany. The strawberry is not indigenous in Japan, but in the mountains there I found a Potentilla in fruit which actually mimicked the Alpine strawberry in the minutest particulars, in its runners, its blossoms, and fruit; but the fruit was simply dry pith, supporting the seeds and retaining its colour without shrinking, or falling from the stalk for weeks — a remarkable case, we cannot say of unconscious mimicry, but of unconscious resemblance. Mimicry in birds is comparatively rare, and still rarer in mammals, which is not surprising when we consider how small is the total number of the Mammalia, and even of birds, compared with the countless species of invertebrates. Out of the vast assemblage of insects, with their varied colours and patterns, it would be strange if there were not many cases of accidental resemblance. A strict application of Wallace's five laws would, perhaps, if all the circumstances were known, eliminate many accepted instances.

As to cases of edible insects mimicking inedible ones, Mr. Poulton admits that even unpalatable animals have their special enemies, and that the enemies of palatable animals are not indefinitely numerous.

Mr. Beddard gives tables of the results obtained by Weismann, Poulton, and others, which show that it is impossible to lay down any definite law upon the subject, and that the likes and dislikes of insect-eating animals are purely relative.

One of the most interesting cases of mimicry is that of the *Volucella*, a genus of Diptera, whose larvæ live on the larvæ of Hymenoptera, and of which the perfect insect closely resembles some species of humble-bee. Though this fact is unquestioned, yet it has recently given rise to a controversy, which, so far as one who has no claim to be an entomologist can judge, proves that, while there is much that can be explained by mimicry, there is, nevertheless, a danger of its advocates pressing it too far. *Volucella bombylans* occurs in two varieties, which prey upon the humble-bees, *Bombus muscorum* and *B. lapidarius*, which they respectively resemble. Mr. Bateson does not question the behaviour of the *Volucella*, but states that neither variety specially represents *B. muscorum*, and yet that they deposit their eggs more frequently in their nests than in the nests of other species which they resemble more closely. He also states that in a show-case in the Royal College of Surgeons, to illustrate mimicry, two specimens of another species, *B. sylvarum*, were placed alongside of the *Volucella*, which they do resemble, but were labelled *B. muscorum*.

But Mr. Hart explains the parasitism in another way. He states that a nest of *B. muscorum* is made on the surface, without much attempt at concealment, and that the bee is a peculiarly gentle species, with a very feeble sting; but that the species which the *Volucella* most resemble are irascible, and therefore more dangerous to intruders. If this be so, it is difficult to see why the *Volucella* should mimic the bee, which it does not affect, more closely than the one which is generally its victim. I do not presume to express any opinion further than this, that the instances I have cited show that there is much reason for further careful observation by the field naturalist, and much yet to be discovered by the physiologist and the chemist, as to the composition and nature of animal pigments.

I had proposed to occupy a considerable portion of my address with a statement of the present position of the controversy on heredity, by far the most difficult and important of all those subjects which at present attract the attention of the biologist; but an attack of illness has compelled me to abandon my purpose. Not that I proposed to venture to express any opinions of my own, for, with such protagonists in the field as Weismann, Wallace, Romanes, and Poulton on the one side, and Herbert Spencer and Hartog on the other, "Non nostrum inter vos tantas componere lites."

So far as I can understand Weismann's theory, he assumes the separation of germ cells and somatic cells, and that each germ cell contains in its nucleus a number of "ids," each "id" representing the personality of an ancestral member of the species, or of an antecedent species. "The first multicellular organism was probably a cluster of similar cells, but these units soon lost their original homogeneity. As the result of mere relative position, some of the cells were especially fitted to provide for the nutrition of the colony, while others undertook the work of reproduction." The latter, or germ-plasm, he assumes to possess an unlimited power of continuance, and that life is endowed with a fixed duration, not because it is contrary to its nature to be unlimited, but because the unlimited existence of individuals would be a luxury without any corresponding advantage.

Herbert Spencer remarks upon this:—"The changes of every aggregate, no matter of what kind, inevitably end in a state of equilibrium. Suns and planets die, as well as organisms." But has the theory been proved, either by the histologist, the microscopist, or the chemist? Spencer presses the point that the immortality of the protozoa has not been proved. And, after all, when Weismann makes the continuity of the germ-plasm the foundation of a theory of heredity, he is building upon a pure hypothesis.

From the continuity of the germ-plasm, and its relative segregation from the body at large, save with respect to nutrition, he deduces, *à priori*, the impossibility of characters acquired by the body being transmitted through the germ-plasm to the offspring. From this he implies that where we find no intelligible mechanism to convey an imprint from the body to the germ, there no imprint can be conveyed. Romanes has brought forward many instances which seem to contradict this theory, and Herbert Spencer remarks that "a recognised principle of reasoning—'the law of parsimony'—forbids the assumption of more causes than are needful for the explanation of phenomena. We have evident causes which arrest the cell multiplication, therefore it is illegitimate to ascribe this arrest to some property inherent in the cells."

With regard to the reduction or disappearance of an organ, he states "that when natural selection, either direct or reverse, is set aside, why the mere cessation of selection should cause decrease of an organ, irrespective of the direct effects of disease, I am unable to see. Beyond the production of changes in the size of parts, by the selection of fortuitously arising variation, I can see but one other cause for the production of them—the competition among the parts for nutriment. . . . The active parts are well supplied, while the inactive parts are ill supplied and dwindle, as does the arm of the Hindu fakir. This competition is the cause of economy of growth—this is the cause of decrease from disease."

I may illustrate Mr. Herbert Spencer's remarks by the familiar instance of the pinions of the Kakapo (*Stringops*)—still remaining, but powerless for flight.

As for acquired habits, such as the modification of bird architecture by the same species under changed circumstances, how they can be better accounted for than by hereditary transmitted instinct, I do not see. I mean such cases as the ground-nesting *Didunculus* in Samoa having saved itself from extinction since the introduction of cats, by roosting and nesting in trees; or the extraordinary acquired habit of the Blackcap in the Canaries, observed by Dr. Lowe, of piercing the calyx of *Hibiscus rosa-sinensis*—an introduced plant—to attract insects, for which he quietly sits waiting. So the lying low of a covey of Partridges under an artificial Kite would seem to be a transmitted instinct from a far-off ancestry not yet lost; for many generations of Partridges, I fear, must have passed since the last Kite hovered over the forefathers of an English Partridge, save in very few parts of the island.

I cannot conclude without recalling that the past year has witnessed the severance of the last link with the pre-Darwinian naturalists in the death of Sir Richard Owen. Though never himself a field-worker, or the discoverer of a single animal living or extinct, his career extends over the whole history of palæontology. I say palæontology, for he was not a geologist in the sense of studying the order, succession, area, structure, and disturbance of strata. But he accumulated facts on the fossil remains that came to his hands, till he won the fame of being the greatest comparative anatomist of the age. To him we owe the building up of the skeletons of the giant *Dinornithidæ*, and many other of the perished forms of the gigantic Sloths, Armadilloes, and Mastodons of South America, Australia, and Europe. He was himself a colossal worker, and he never worked for popularity. He had lived and worked too long before the Victorian age to accept readily the doctrines which have revolutionised that science, though none has had a larger share in accumulating the facts, the combination of which of necessity produced that transformation. But, though he clung fondly to his old idea of the archetype, no man did more than Owen to explode the rival theories of both Wernerians and Huttonians, till the controversies of Plutonians and Neptunians come to us from the far past with as little to move our interest as the blue and green controversies of Constantinople.

Nor can we forget that it is to Sir Richard's indomitable perseverance that we owe the magnificent palace which contains the national collections in Cromwell Road. For many years he fought the battle almost alone. His demand for a building of two stories, covering five acres, was denounced as audacious. The scheme was pronounced foolish, crazy, and extravagant; but, after twenty years' struggle, he was victorious, and in 1872 the Act was passed which gave not five, but more than seven acres for the purpose. Owen retired from its direction in 1883, having achieved the crowning victory of his life. Looking back in his old age on the scientific achievements of the past, he fully recognised the prospects of still further advances, and observed, "The known is very small compared with the knowable, and we may trust in the Author of all truth, who, I think, will not let that truth remain for ever hidden."

I have endeavoured to show that there is still room for all workers, that the naturalist has his place, though the morphologist and the physiologist have rightly come into far greater prominence, and we need not yet abandon the field-glass and the lens for the microscope and the scalpel. The studies of the laboratory still leave room for the observations of the field. The investigation of muscles, the analysis of brain tissue, the research into the chemical properties of pigment, have not rendered worthless the study and observation of life and habits. As you cannot diagnose the Red Indian and the Anglo-Saxon by a comparison of their respective skeletons or researches into their muscular structure, but require to know the habits, the language, the modes of thought of each; so the mammal, the bird, and even the invertebrate, has his character, his voice, his impulses, aye, I will add, his ideas, to be taken into account in order to discriminate him. There is something beyond matter in life, even in its lowest forms. I may quote on this the caution uttered by a predecessor of mine in this chair (Professor Milnes Marshall): "One thing above all is apparent, that embryologists must not work single-handed; must not be satisfied with an acquaintance, however exact, with animals from the side of development only; for embryos have this in common with maps, that too close and too exclusive a study of them is apt to disturb a man's reasoning power."

The ancient Greek philosopher gives us a threefold division of the intellectual faculties,—φρόνησις, ἐπιστήμη, σύνεσις—and I think we may apply it to the subdivision of labour in natural science: φρόνησις, ἡ τὰ καθ᾽ ἕκαστα γνωρίζουσα, is the power that divides, discerns, distinguishes—*i. e.*, the naturalist; σύνεσις, the operation of the closet zoologist, who investigates and experiments; and ἐπιστήμη, the faculty of the philosopher, who draws his conclusions from facts and observations.

The older naturalists lost much from lack of the records of previous observations; their difficulties were not ours, but they went to Nature for their teachings rather than to books. Now we find it hard to avoid being smothered with the literature on the subject, and being choked with the dust of libraries. The danger against which Professor Marshall warns the embryologist is not confined to him alone; the observer of facts is equally exposed to it, and he must beware of the danger, else he may become a mere materialist. The poetic, the imaginative, the emotional, the spiritual, all go to make up the man; and if one of these is missing, he is incomplete.

I canot but feel that the danger of this concentration upon one side only of nature is painfully illustrated in the life of our great master, Darwin. In his early days he was a lover of literature, he delighted in Shakespeare and other poets; but after years of scientific activity and interest, he found, on taking them up again, that he had not only grown indifferent to them, but that they were even distasteful to him. He had suffered a sort of atrophy on that side of his nature, as the disused pinions of the Kakapo have become powerless—the spiritual, the imaginative, the emotional, we may call it.

The case of Darwin illustrates a law,—a principle we may call it,—namely, that the spiritual faculty lives or dies by exercise of the want of it, even as does the bodily. Yet the atrophy was unconscious. Far was it from Darwin to ignore or depreciate studies not his own. He has shown us this when he prefixed to the title-page of his great work the following extract from Lord Chancellor Bacon: "To conclude, therefore, let no man, out of a weak conceit of sobriety, or an ill-applied moderation, think or maintain that a man can search too far, or be too well studied in the book of God's word, or in the book of God's works, divinity or philosophy, but rather let men endeavour an endless progress or proficience in both." In true harmony this with the spirit of the father of natural history, concluding with the words, "O Lord, how manifold are Thy works, in wisdom hast Thou made them all, the earth is full of Thy riches."

BRITISH ASSOCIATION REPORTS.

Following the Presidential Address in the Biological Section —which owing to the regrettable illness of the President, the Rev. Canon Tristram, was read in his absence—several reports of much interest to zoologists were presented.

Marine biological investigations.—Investigations made at the laboratory of the Marine Biological Association at Plymouth formed the subject of a report presented by the committee on this subject, including contributions from Mr. F. W. Gamble, B.Sc., on "The Turbellaria of Plymouth Sound," by Mr. Edgar J. Allen, B.Sc., on "The Larvæ of Decapod Crustacea," and by Mr. Gregg Wilson, M.A., B.Sc., on "How Fish find Food."

The luminous organs in Cephalopoda.—An interesting paper was read by Mr. W. E. Hoyle, illustrated by means of a number of limelight pictures, describing the luminous organs of certain cephalopods.

The Marine Zoology of the Irish Sea.—Professor W. A. Herdman presented a valuable report, illustrated with maps and diagrams, from a committee appointed to explore the region of the Irish Sea which lies around the Isle of Man. The committee consisted of the late Mr. George Brook, Professor A. C. Haddon, Mr. W. E. Hoyle, Mr. J. C. Thompson, Mr. A. O. Walker, and Professor W. A. Herdman. The greater part of the area has never previously been thoroughly explored, and some parts are still unexamined. It is a very interesting region from the great diversity of shore, depth, and bottom which it presents. The Isle of Man is connected with Lancashire by a broad plateau under 20 fathoms in depth. A considerable portion of the work has been done in the broad southern extension which lies between Liverpool and the Calf of Man, and gives depths of from 20 to 40 fathoms. In the shallower water around the coasts there is great difference in the physical conditions, and in the fauna of different regions: for example, the sandbanks and flat expanses of mud off the Lancashire coast are very different in every way from the more varied ground off the rocky southern shore of the Isle of Man. But even the seemingly uninteresting sandy wastes of Lancashire present many curious facts and problems to the marine biologist. It is found that on the estuarine flats round Hilbre Island, as Lindstrom suspected to be the case on the coast of Gothland some years ago, the very abundant *Hydrobia ulvæ* lays its eggs upon its neighbours' shells, probably as being the largest and most stable objects among the shifting sand-grains around it. This supposed barren region, too, is of great economic importance as a nursery for young food-fishes. Eight expeditions were made between September and June last, and as a result about 1000 species of marine animals were collected and identified. Of these 38 are new to the British fauna, 223 are new to the particular district, and 17 are new to science.

Seals and Whales.—Mr. W. S. Bruce contributed some notes on Seals and Whales seen during a voyage to the Antarctic regions in the steam-whaler 'Balæna' from Dundee in 1892–93. "After a boisterous passage of over 100 days, the ship encountered the first iceberg on December 16th, 1892, in 59 deg. 40 min. S., 51 deg. 17 min. W. We continued in a more or less southerly course, passing to the east of Clarence Island. Danger Islets were sighted and passed on December 23rd, and on Christmas Eve we were in the position Ross occupied on New Year's Day, 1843. Until the middle of February we remained roughly between 62 deg. S. and 64 deg. 40 min. S. and 52 deg. and 57 deg. W., the western limit being Terre Louis Philippe and Joinville's Land. All the land seen was entirely snow-clad except on the steepest slopes, which were of black, apparently igneous, rocks. At least three kinds of Seals were observed. These were all true Seals; no Fur Seals were seen. They were the Sea-leopard (*Stenorhynchus leptonyx*), Weddell's false Sea-leopard (*Leptonyx Weddellii*), and a creamy-white Seal, probably the Crab-eating Seal (*Lobodon carcinophaga*). There were two others, which were possibly younger forms of Sea-leopard and Crab-eating Seals respectively. The latter, instead of being white, was mottled grey, but similar in form and size to, and often found among, the white Seals. In December all the Seals were in very bad condition, thinly blubbered and grievously scarred; the females as well as the males. There was no marked preponderance in the number of the females. During January their condition improved, and by February they were heavily blubbered, and free from scars. Loving the sun, they lie on the pack-ice all day, digesting their meal of the previous night, which consists of fish or small Crustacea, or both; Penguins also are occasionally their victims, and stones were found in their stomachs. By February the embryo is well developed, gestation probably beginning in December. It is extremely regrettable that it was during this period the indiscriminate slaughter took place, almost all the females towards the end of January and February being with young. All the Seals were found on the pack-ice; the Sea-leopard was on the outer-most streams, and was most frequently to be found singly, though two or three might be on one piece of ice, but seldom more. Weddell's false Sea-leopard was very rare, only four of them having been seen. The creamy-white Seal and pale mottled grey ones were in greatest abundance: these were found in fours, fives, or even tens—the greatest number seen on one piece was forty-seven. On one occasion some Seals were found on a tilted berg; so high was the ledge above the water-level that a man with difficulty clambered up and secured the prey. This illustrates their great power of jumping from the water on to the ice. They were seen rising about nine feet above water, and cover distances of fully twenty feet in length. No trace was seen of any Whale resembling the Bow-head or Greenland Black Whale (*Balæna mysticetus*), which Ross reported to have seen in very great numbers. There were, however, Humpbacks, Finners, Bottle-noses, and Grampuses.

Protection of Wild Birds' Eggs.—Under the presidency of Prof. Newton, in the absence of Canon Tristram through illness, Dr. C. Vachell presented the report of a Committee on the "Legislative Protection of Wild Birds," which ran as follows:—

Your committee beg leave to report that early in the present session, a Bill to amend the Wild Birds Protection Act, 1880, was brought into the House of Commons by Sir Herbert Maxwell, M.P., and others, and that on April 13th it was ordered by the House to be printed. Thereupon your committee gave this Bill their careful attention, and found that its main clause contained a provision for the protection of wild birds' eggs. In the opinion of your committee, however, this provision was framed on a principle that appears to them to be mistaken, in that it sought to effect the desired object by empowering local authorities to name the species the eggs of which were to be protected, thus requiring in every case of prosecution proof of identity, which in the majority of cases would be difficult, if not impossible, to supply. Nevertheless, the Bill met with favourable acceptance in the House of Commons, and with some very trifling alterations only, and without any discussion of its principle, passed the third reading, and was sent up to the House of Lords on May 2nd. In the House of Lords the chief objection to the Bill, which had already been observed by your committee, was, among others, prominently brought forward by several speeches in a debate on the second reading, June 14th, and accordingly a series of amendments were introduced and carried when the Bill was in Committee, on June 16th. In almost every point these amendments, and especially one that provided that protection should be given to birds which most required it, by empowering local authorities to name areas in which for a given time the taking of eggs should be wholly prohibited, accorded with the opinion at which your committee had previously arrived. Subsequently the Bill was further amended by the Standing Committee of the House of Lords, and, having been read a third time, was sent back to the House of Commons for its approval of their Lordships' amendments.

These your committee, after duly considering them, had hoped would be at once accepted by the House of Commons; but, on August 21st, on the motion of Sir Herbert Maxwell, it was agreed that consideration of them should be adjourned for three months, and therefore the fate of the Bill remains doubtful.

In view of the uncertainty thus existing your committee would recommend their reappointment on the same terms as before.

Dr. Vachell observed that unfortunately serious differences of opinion existed as to the way in which birds' eggs should be protected. Everybody was agreed it was necessary to extend the protection. It was thought by some that the County Council should be moved to prevent the taking of particular eggs in particular places at particular times of the year. But some ornithologists told them that that was impossible, as many of the eggs were so similar that in a court of law it would be impossible to swear to a particular egg being the egg of a particular species. It had, therefore, been suggested that if a particular species were declining, and were known to frequent a particular place, the County Council should have the matter represented to them, and be called upon to protect that restricted area. The principle was that they should protect areas, and not species.

Professor Newton, who wound up the discussion which followed, alluded to the practical impossibility of ever getting a conviction, owing to the difficulty of distinguishing between the eggs of one species and another.

THE "RUSSET-PATED CHOUGH" OF SHAKESPEARE.

By Professor Newton, M.A., F.R.S.

The Editor having courteously sent me an early copy of his latest remarks on this subject (*anteà*, p. 332), I think it only fitting to offer a few observations upon them, in support of the adherence I have hitherto given to Mr. E. T. Bennett's explanation of the Shakespearian passage. I quite agree with the Editor as to the need, from his point of view, of establishing the first three positions he takes up. As to the fourth, I think it immaterial and arises from a misconception; but there is a fifth, which is very material and is overlooked by him.

(1). That "Chough," without qualification, was a name used by many writers indiscriminately for "Cornish" Chough and for Daw, no proof is required; but in this particular passage the word is qualified by an epithet. Our business is to know what that epithet means.

(2). I admit that no other instance of "pated" being used by Shakespear can be found, or can be adduced from any other author, so far as I can learn. The word is thus ἅπαξ λεγόμενον, and every scholar knows the difficulties that such words present. In the corrupt condition of all the old editions of the poet's works no one can vouch for the spelling being that of the author; or, if it were, whether it would be any the better for that. People are apt to forget that he lived when there were no settled rules for spelling English, and that he himself, according to the few autographs which have come down to us, had several ways of spelling his own name. We are quite at liberty to read "patted" or "pated" as we think fit, and the fact that the early printers used the latter form is no proof of its being the right one. Indeed, seeing that, as Mr. Harting says, Shakespear brings in the word "pate" fourteen times (and of course about *pate* in the sense of head there is no doubt), that might at once account for the printer using "pated" in this case, for even a printer (with all respect to him) is not wholly free from bias. But there is something more to be said of this *pate.* I write with deference, but I think it was not the custom in Shakespear's time, as it has since become, to coin an adjective in form of a participle from a purely English noun, such as I take *pate* to be, though as Prof. Skeat tells us, it is of French origin. Where is the French *paté* = "headed"? When that is found, and Littré is silent respecting such a word,[*] it will be conceivable that Shakespear used *pated* = headed. But in the French *patté,* well known in heraldry,[†] if not in other ways, he had the word at hand.

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[*] Littré says that in old French *patte* was sometimes spelt *pate.*

[†] It is rather beside the question, but I take leave to remark that a cross *paté* or *pattée* is not quite the same as a Maltese cross.

(3). That "russet" originally meant red or reddish, no one can dispute. That in later times it may mean some other colour is very likely equally beyond contention. The point which, it seems to me, if I may so say, that Mr. Harting has missed is, what did it mean in Shakespear's time. No later writer is here any authority. Mr. Harting shows that Skinner in 1671, more than fifty years after the poet's death, had only one meaning for it—red, or at least reddish. If it can be shown that "russet" meant grey in Shakespear's time, the case will be half established. Failing that, none of the other authors cited help it; indeed, many of them tell against it. The quotation from Mant evidently refers to the rusty tint put on by dead-ripe grass; Gisborne's "russet" fern is the plant turning reddish-brown; Somervile's "russet" is the colour of the Hare's head, which in England is certainly not grey, whatever the Daw's be, and so on of the rest. Milton indeed has his "russet lawns" in contradistinction to "fallows grey."

But now comes the consideration of "russet," not as a colour but as a woven stuff, whether of woollen, cotton, or what, matters not; though no doubt the fabric was originally named from its colour, and when, as certainly happened, the original meaning of the word was changed, and some other dye was used for the cloth, another adjective would naturally be applied to indicate its colour. Thus we have the expressions "grey russet" and "russet grey," meaning grey stuff; but evidence is, as I have said, wanting* to show that this use existed in Shakespear's day.†

(4). Here I would submit that Mr. Harting's interpretation of the passage is at fault. "Many in sort" means many in company —the use of *sort* in this sense is common enough, and instances are too numerous to cite—"a sort of Mallards" is proverbial and will be sufficient.

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* There is the doubtful exception quoted from Turbervile, in which there is nothing to show whether "russet pyle" refers to the surface resembling the fabric "russet" and not to the colour. Certainly such Stags' horns as I have seen "in the velvet" cannot strictly be called *grey* in the modern sense.

† We have exactly the converse in *scarlet*, which comes from a Persian word signifying cloth, and because that cloth was of a particular colour, the word came to mean the colour. So with "plaid." Originally a piece of stuff used to wrap round the body, which stuff was woven in a checked pattern (tartan), *plaid* has come to mean anything that shows a pattern with crossing stripes.

(5). But I said I would add a fifth position to Mr. Harting's four, and this seems to me the only one which concerns the ornithologist. I must quote the whole passage:—

> ". . . . . . When they him spy,
> As wild geese that the creeping fowler eye,
> Or russet-pated Choughs, many in sort,
> Rising and cawing at the gun's report,
> Sever themselves and madly sweep the sky,
> So, at his sight, away his fellows fly."

Here we have the fowler creeping on the ground and shooting at a numerous flock of *Corvidæ* as they rise before him, and fly dispersedly in alarm over his head. Which part of each bird is then most visible to him? its nape or its legs? Let any one ask himself whether in these conditions the grey collar of the Daw, seen from beneath and against the sky, would be perceptible, and whether the red feet of the Cornish Chough would not be characteristic if not conspicuous.

SUPPLEMENTARY NOTE.

By the Editor.

In my article on this subject in the last number, reference was made (p. 336) to Dr. Johnson's statement that Sir Isaac Newton had employed the term *russet* in the sense of gray, but at the time of writing I was unable to find the particular passage referred to. Thanks to the courtesy of a correspondent, Mr. G. R. Murdoch, of Kendal, I am now enabled to quote it. It occurs in the 'Opticks' (4to, London, 1704; Book II. p. 89), and is as follows:—"This white spot was immediately encompassed with a dark grey or *russet*, and that darkness with the colours of the first iris."

From Sir Isaac Newton's point of view, therefore, a russet-pated Chough might be a grey-headed Jackdaw.

NOTES AND QUERIES.

Death of the Rev. Leonard Blomefield, M.A., F.L.S.—A venerable link with the past generation of naturalists has been severed by the death of the Rev. Leonard Blomefield (formerly Jenyns), which took place at Belmont, Bath, on Sept. 1st, in the ninety-third year of his age. Having led a life of retirement for many years past, it will probably surprise most of our readers to learn that the author of the 'Manual of British Vertebrate Animals,' published so long ago as 1835 (before many of us were born), has only just passed away. In our next number we hope to give some account of his life and labours in the cause of Zoology.

MAMMALIA.

Destruction of Wild Beasts in India.—A resolution by the Chief Commissioner of the Central Provinces in India on various reports relating to the damage caused by wild animals and the extermination of the latter, contains some observations of more than usual interest. Amongst the animals killed in 1892 for which rewards were paid were 274 Tigers, 442 Panthers, 131 Bears, and 85 Wolves. In the past four years over 1000 Tigers, 2000 Panthers, 500 Bears, and 300 Wolves have been destroyed. Last year 317 persons were killed by wild beasts in the provinces, while the number of deaths from snake-bite was 999. The destruction of cattle is astounding, showing an increase of about 1200 over the previous year. Sir Anthony Macdonnell accounts for this by saying that during his recent tour he noticed a great scarcity of deer in the jungle tracts. They are being cleared out by the native huntsman, and the decrease in the natural prey of the Tigers and Panthers is marked by an increased loss of cattle. On the subject of rewards for the destruction of wild animals, the experience of the Central Provinces seems to show that the system of offering special rewards for the destruction of particular animals or classes of animals is a sound and effective one. The resolution mentions instances of this. In July last it was decided to increase the reward from 10 rupees to 50 when it was proved the animal was a man-eater; again, a special reward of 50 rupees was offered for the destruction of a Bear which had been doing much damage in the Balaghat jungles, while 300 rupees was offered for a man-eating Tiger in the Chanda forests. It is said that special rewards offered in March last year for the destruction of Wolves in the Saugor district had the result of reducing the number of deaths attributed to Wolves in that district from eleven to one. The Chief Commissioner also notices the localisation in a few districts of most of the deaths caused by Tigers and Wolves. For instance, out of 98 deaths caused by Tigers in the Central Provinces in 1892, 68 occurred in the adjacent districts of Chanda, Hoshangabad, and Raipur.

BIRDS.

Black Guillemot on the Solway Firth.—My son shot a specimen of the Black Guillemot on the Solway Firth on August 1st. It was in immature plumage, and has been carefully preserved. This is only the second example of this species which I have seen on the Firsth during the last seven years. It is a rare bird with us.—J. J. Armistead (Solway Fishery, Dumfries).

Reported occurrence of the Black Woodpecker in the New Forest.—In answer to your questions, I feel perfectly certain that I saw a pair of Great Black Woodpeckers, *Picus martius*, on a beech at Stony Cross on the afternoon of May 14th, 1889. In my mind's eye I can see them now. Oh, the insult of suggesting that we might have mistaken a pair of Jackdaws for them! You must know that we were brought up in the country, where Woodpeckers abounded, at any rate the Green and the Greater Spotted, and to the Lesser Spotted I was introduced twenty years ago—to a dead specimen, at least, on a keeper's gibbet. But, to return to our friends of Stony Cross, I am bad at calculating distances, but I should say they were not more than twenty yards off. We watched them for some time, and I made a little sketch of them in my book. If only we had a glass, I might have sworn to every feather. I remember how tame they seemed, or at least how oblivious of our presence. . . . . I found the date immediately in John's 'British Birds in their Haunts,' but I have also the entry in my journal, "Saw a pair of Black Woodpeckers close to," &c.—Mrs. Anderson (Lea Hall, Gainsborough).—[Communicated by Rev. J. E. Kelsall.]

Lesser Whitethroat breeding in Carnarvonshire.—On the 27th May last we found a nest of the Lesser Whitethroat, *Sylvia curruca*, containing a single egg, near Abersoch, on Cardigan Bay. It was placed in a gorse-bush, about a foot above a small stream. The nest was neatly built of grass, lined with finer bents and a little horsehair, and was unusually deep for this species. The bird slipped away on our approach, and sneaked through the undergrowth, so that we were not able to see it clearly. Is this not rather far west for the Lesser Whitethroat to breed?—T. A. Coward (Bowdon).

[Its occurrence in Breconshire in 1886 was noted by Mr. Cambridge Phillips. See 'Zoologist,' 1886, p. 418.—Ed.]

Black Tern in Warwickshire.—On August 12th an immature Black Tern had taken up its abode on Powell's Pool, the largest of six pools in Sutton Coldfield Park. It spent most of its time resting upon a stump that projected about a foot out of the water. When upon the wing it frequented that part of the pool nearest the large reed-bed and marsh, and had little fear of the fishermen immediately below, occasionally being mobbed by a few Swallows, to whom it paid but little heed. On August 20th Mr. Oldham and I spent nearly two hours watching its movements until it took up its usual position for the night on the above-named stump, which stood some hundred yards from the side of the pool. On August 22nd Mr. Oldham paid the pool another visit, and, taking advantage of a pleasure steamer, passed within a few feet of the post on which the bird was resting. Altogether it made a stay of at least eleven days with us.—J. S. Elliott (Dixon's Green, Dudley).

Black Stork in Middlesex.—An adult male Black Stork, *Ciconia nigra*, was shot at Northolt, near Harrow-on-the-Hill, on the 25th July last. It had been seen about for six weeks—so it was said by the man who shot it—and was said to have been killing and eating young chickens. It was very shy, and when approached would fly to the top of a haystack, where it generally roosted, and where it was eventually shot. It showed no traces of captivity, but that, of course, does not prove that it had not escaped from confinement. I enquired of Messrs. Bailey, in Mount Street, but learnt that no such bird had been lost from their aviaries. On examination, it appeared that it was moulting the third and sixth primaries. It measured in length 3 ft. 4½ in.; expanse of wing, 5 ft. 11 in.; wing, 1 ft. 9 in., and is now in the hands of Mr. J. R. Goshawk, of Harrow, for preservation. It is to be regretted that such rare feathered visitors should not be allowed to remain unmolested and receive that encouragement and protection which is afforded to the White Stork in Holland and Germany.—R. Meinertzhagen (Harrow-on-the-Hill).

Lesser Redpoll nesting in Dorsetshire.—It may interest readers to know that a pair of Lesser Redpolls, *Linota rufescens*, have nested in a prune tree in the garden here this year. The young birds are fledged. I have one egg which was left in the nest, and consider it a valuable addition to my collection. Surely its nest in Dorsetshire must be a rare occurrence.—A. J. Bengough (Upton, Poole).

[Mr. Mansel Pleydell, in his 'Birds of Dorsetshire,' states that this bird occasionally breeds in Dorset, and mentions a nest with eggs found at Thorncombe, Blandford.—Ed.]

Baillon's Crake in Dorsetshire.—On June 1st a specimen of this rare bird was captured in the stable of Mr. Hardy's builders' yard at Swanage, within a hundred yards of the sea, and was given to me to have preserved. I have received a letter from Mr. Hart, of Christchurch, who has seen the bird, and he authorises me to say that it is undoubtedly Baillon's Crake. The characters which distinguish it from the Little Crake are its smaller size (scarcely seven inches in length), its olive legs and feet, and the white outer web of the first primary. It is somewhat singular that another rare bird, the Little Bittern (a specimen of which I recorded in 'The Field' as having been taken near here seven years ago), was also captured in a stable or cowshed. This bird (an adult male) is still on view in the town.—JAS. ANDREWS (Swanage).

[The above information is just what was required to settle the question of identification, and we take this opportunity of remarking that it would be well if correspondents, who do not claim to be experts, when reporting the occurrence of a rare species, would state either their authority for the name, or give very briefly the distinguishing characters upon which they rely in their determination of the species.—ED.]

REPTILIA.

Dimensions of the Adder.—On July 14th I killed an Adder, *Pelias berus*, near Ascot, of the following dimensions (are they not exceptionally large?):—Length, 21⅞ in.; circumference in three places, 2¼ in., 2⅝ in., 2½ in.; head, 11/16 in. across; sixty-three black diamond marks on back, 148 scales on stomach. It was in the middle of casting its skin, and had got it back from its head. I took all the old skin off, and, as it was not damaged in the slightest by the blow I gave it on the head, it presents a grand appearance in spirits.—G. V. CHARLTON (Somerset House, Ascot).

[The dimensions are above the average, but we have seen one killed in West Sussex that was 2 in. longer.—ED.]

I have one which I killed some years ago in Surrey, and which measured, before being put into spirits, 26 in.; and in the same year, and the same locality, I saw two others killed, both of which were 24 in. in length. These are no doubt exceptionally long, but I fancy are by no means the "longest on record." It would be interesting to know the dimensions of the largest which has hitherto been recorded. I may add that my 26 in. specimen was measured by myself before it was quite dead.—GEO. E. LODGE (5, Verulam Buildings, Gray's Inn).

I have a preserved specimen of the Adder in my collection which measures 28 in. in length and 3½ in. in girth. It is a female, and was killed by my brother near Grantley Hall, the residence of Lord Grantley. A male was killed at the same time, but it was not quite so large, and was so much injured that it was not worth preserving.—JAMES CARTER (Burton House, Masham).

American Fur-Seals: Correction.—On page 360, line 7, the number of skins of Fur-Seals received from America in 1891, according to Mr. Poland, should be 125,731, not 725,731 as printed.

NOTICES OF NEW BOOKS.

The Amphioxus and its Development. By Dr. B. Hatschek. Translated and edited by James Tuckey, M.A. 8vo, pp. 181, plates i—ix. London: Swan Sonnenschein & Co. New York: Macmillan & Co. 1893. (*Introductory Science Text-books*).

This work is translated from the German 'Studien ueber Entwicklung des Amphioxus' of Hatschek, published in 1882 in the somewhat inaccessible 'Arbeiten' of the Vienna and Trieste Zoological Institute. Though it professes to be edited as well by the translator, we find no reference in his Preface to the nature of his labours; and in this respect an occasional footnote, giving a scientific term introduced since the appearance of the original edition, seems to be the only editorial work accomplished. The translator writes clear English wherever he has succeeded in fully grasping the meaning of the German; many obscure sentences are generally to be understood by retranslation into the original language: thus "Some control should be had over such observation [of living objects] through investigation of preserved material to be undertaken by day" becomes intelligible when we see that "should" represents *sollte* (= was necessary). But it requires a reference to the original to make sense of the bull in the italicised words of the following passage:—"In this way no ciliated cells are to be seen in the Amphioxus, *nor in the grown animal even during development, but only flagellate cells.*" Mr. Tuckey seems unfamiliar with scientific terminology in English as well as German, and to have been badly helped in this respect by those friends whom he thanks in the Preface. *Fibrillum*, plural *fibrilla*, is neither English nor Latin. *Carmin* is as unknown to the English chemist, as *Beal* is to our literature. Judging from the readiness with which competent scientific men will read a work of this character for review, it would seem an easy matter to retain their services for reading the proof-sheets; and it is not creditable to English publishers that they so frequently allow translations of scientific works to go forth to the world with so many avoidable blemishes. In a widely circulated book on Fermentation, translated from the French, we are told that French and Germans are satisfied to drink beer made with "inferior yeast";* there is nothing so bad in the present work; and so far Mr. Tuckey is to be congratulated.

The title of this English translation is somewhat misleading; it only contains an account of the breeding habits of Amphioxus at Faro, near Messina, and of the development of the fertilised egg as far as the formation of the mouth and first gill-slit. The account given is admirably systematic and clear, and the figures are good and numerous, though in some cases too small to be readily interpreted, for they are reduced to half the diameter of the originals. But to place on the right-hand cover the word "Amphioxus" is seriously to mislead the unwary. A good text-book under such a title would give, in a continuous form, the anatomical structure and full development of Amphioxus, as completed by Prof. Ray Lankester's researches, and those of his pupils; and this is so great a desideratum that our satisfaction in obtaining this book in English is marred by the fear that it may preclude the appearance of the other.

M. H.

The Great Sea Serpent: an historical and critical treatise. By A. C. OUDEMANS. Royal 8vo, pp. i—xv, 1—592; with 82 Illustrations. Leiden: E. J. Brill. London: Luzac & Co.

THIS voluminous treatise is in many respects disappointing. The subject is a good one, and, with the materials so industriously collected by the author, ought to have furnished something more satisfactory in the way of result. Mr. Oudemans seems to have collected every printed report and story of a Sea Serpent that he could lay his hands upon, good, bad, and indifferent, and arranged them chronologically, from the 'History' of Olaus Magnus, A.D. 1555, down to a Dutch weekly newspaper ('De Amsterdammer') of July, 1890. While he was about it, seeking for observations by ancient authors, he might as well have quoted Pliny (Hist. Nat. lib. viii. cap. 14), but he would have done better had he omitted more than half the stories he has reprinted, as being quite unworthy of credence or consideration. To have taken only the most authentic accounts of the appearance of strange marine forms supposed to have been Sea Serpents, to have examined them critically from the zoologist's point of view, and to have compared them one with another, to show their consistencies and inconsistencies, would have been an interesting and useful piece of work, but the remarkable conclusions at which Mr. Oudemans has arrived show that he cannot be regarded as a safe guide, and the reader so far from accepting his conclusions, must be left to form his own opinion of the value of the various accounts presented to him.

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* "Bottom yeast" is the correct expression for "levûre inférieure."

Although Mr. Oudemans has never seen a "Sea Serpent" himself, nor examined or dissected any portion of one procured by anybody else, he has no hesitation in evolving an animal from the descriptions which he has collected, and bestowing upon it a scientific name! In Nov., 1881, in the first number of the 'Album der Natur,' treating of the probability of the existence of a Great Sea Serpent, he examined the characters ascribed to it by various writers, and came to the conclusion that it "must be a mammal with four flappers, a long neck, and a long and pointed tail, and that the position of this marine mammal is between Dolphins and Pinnipeds." Accordingly he proposed to name it *Zeuglodon plesiosauroides* (p. 445). After ten years' consideration he is apparently unwilling to abandon his position, and after studying the law of priority, while considering the rules for zoological nomenclature, he has come to the conclusion (p. 546) that the oldest name for the genus is *Megophias*, of Rafinesque, and that the only name to be given to the Sea Serpent is that of *Megophias megophias* (Raf.) Oudemans!

Although we are unable to accept his conclusions, we fully recognise the amount of labour bestowed upon this compilation, which, it must be allowed, is extremely entertaining. A little more care in the revision of the proof-sheets, especially in regard to the spelling of English proper names, would have been well bestowed.

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